that this country is fast losing its old industrial supremacy through sheer lack of the scientific knowledge which other countries are turning to such practical account? But it is not on these grounds we would urge the leading scientific societies in our great provincial towns to follow the admirable example set by Birmingham. Scientific research, for its own sake, is a worthy and ennobling pursuit, blessing those that give as well as those that receive the funds for carrying it on, when these are given in a generous spirit and with a discriminating hand. We feel quite sure the Birmingham Philosophical Society would have not only little difficulty in raising the modest sum with which they have ventured to start, but that the wealthy Birmingham manufacturers, and probably even the Birmingham Corporation, will see it to be to their best interests to make the fund a permanent one, and so increase it as to produce wide and substantial results. The example, it is to be hoped, will be followed by other provincial towns, as Manchester, Liverpool, Newcastle, and Glasgow, all of which have reputable philosophic or other similar societies, plenty of money to spare, and everything to gain and nothing to lose by such a wise and noble use of it.

May we not also hope that the example set by this provincial Society will strengthen the weakness of knee which, in the opinion of many, the Royal Society has displayed in its administration of the fund which Government has committed to its care for the endowment of research? At Birmingham the endowment becomes an honour, and not an alms to be sued for on the "proper form," and it is not frittered away so as to miss the real object of the creation of the fund. That some such fund is necessary seems to us clearly proved, if further proof were needed, by the action of the Birmingham Society; and the Royal Society will show itself scarcely worthy of its position as the leading Society of the kingdom and the only Society which demands original research as a condition of admission, if through feebleness or any false sense of dignity it should allow the modest sum it now administers to lapse from its hands. It need not fear that in administering this fund, and in taking all the trouble that must be taken to do so wisely and honestly, it sustains any loss of prestige. There are certain things with which to meddle would certainly be undignified on its part; but in doing work of this kind it seems to us it is performing a proper function.

Perhaps nothing would sooner convince our ignorant and one-sided politicians of the reality of science, and of the necessity for its national recognition than efforts similar to that begun at Birmingham, carried out in all our great industrial centres. In the somewhat humiliating agitation now being carried on all over the country we hear much from both sides of the country's highest welfare; and yet not a single statesman of them all ever gives a hint that he knows what science really means, far less what important national issues depend upon the results of its cultivation. Let our great municipalities take the matter up as Birmingham has done, and we are confident that while much will thus be done for the promotion of scientific research throughout the country, their action will not be without its effect upon the Government. For while such action in the provinces is in the highest degree desirable and laudable, it is no more a substitute for the national recognition of science than municipal government is a substitute for a central administration.

Meantime the Birmingham Philosophical Society, whatever may be the final result of its enterprise, will be entitled to hold an honourable place in the annals of English science.

ECLIPSE OBSERVATIONS

Observations made during Total Solar Eclipses. Collated by A. C. Ranyard, M.A., Sec. R.A.S. *Memoirs* of the Royal Astronomical Society, vol. xli., 1879.

THE idea of collecting different accounts of the same eclipse, and breaking them up, so that all descriptions of one and the same phenomenon should be found side by side, first originated with the Astronomer-Royal, who began to collect all accounts he could procure of the eclipse of 1860. As pressure of work prevented him from carrying out his idea, Mr. Cowper Ranyard took it up at his suggestion and gradually extended the plan, so as to include all the more important physical observations which have ever been recorded during total solar eclipses. This enormous work has now been published in a volume of nearly 800 pages, and there cannot be two opinions as to its usefulness and value. It must, however, be borne in mind that this is a mere work of compilation, and the reader who expects to find a general and correct account of the conclusions to be drawn from the observed phenomena and the results which have actually been arrived at, will be bewildered rather than instructed by the perusal of the book. The volume is simply intended to classify the observations which have been made, and not to discuss them. A good discussion is very much wanted, but it could hardly have been made by a single man, and certainly not without consulting the chief authorities on the subject. It is of course impossible to avoid altogether reference to theories which have been proposed, and their comparison with observations for which they are supposed to account, but Mr. Ranyard has acted in this respect with commendable self-restraint, and whenever he departs from his general rule, he only makes us feel how grateful we ought to be to him, that he has not more often indulged in such vague, confused, and unsatisfactory discussions as here and there disfigure the book. As it is, it will not be difficult to draw a pen through all statements involving debateable matters, and we shall then have a volume which will do credit to its author and to the Society which has published it.

In order to gain an idea of the great variety of observations which are dealt with in the volume, we have only to look over the table of contents.

The first chapters contain accounts of phenomena of minor importance, yet of considerable interest. Most of these can also be observed in partial eclipses, such as the occultation of sun-spots by the moon, the darkness of the moon compared to sun-spots, the fringe round the moon's limb, &c.

Chapter IX. contains an account of the remarkable moving shadow-bands which have been observed just before and after totality. There can be no doubt that these shadows originate in our own atmosphere; but whether the currents which give rise to them are produced by the chilling effect of the eclipse, or whether they are always present, but only cast a visible shadow when the solar light is confined to a narrow crescent, we do not know. These bands were noticed a short time ago, without an eclipse, in Lord Lindsay's observatory, and were then produced by the sun disappearing behind the mountains. The eleventh chapter treats of the brushes of light apparently emanating from the cusps of the sun during the partial phase. Prof. Stokes has offered an explanation of this phenomenon, and the explanation seems to agree very well with the drawings given by some of the observers. It cannot, however, be reconciled, we believe, with the description given by Mr. Brett, who observed them through his telescope and called them "exquisitely defined."

The subjects of greatest interest and importance of the volume are contained in the three last chapters, which form about two-thirds of the whole volume. They treat of "Polariscopic Observations," "Spectroscopic Observations," and "Photographs and Drawings of the Corona." A good part of the chapter on polarisation may serve as an example of what the book might have been, had Mr. Ranyard more frequently indulged in theoretical speculations. We cannot of course go into any detailed criticism, but shall give two examples in order to justify our remarks. In the first place, we think that Mr. Ranyard has treated the observations which relate to the polarisation of our atmosphere in the parts of the sky occupied by the corona, in a rather careless manner. It appears that one observer makes the polarisation vertical, one horizontal, and seven leave it doubtful whether the polarisation was vertical or horizontal. But on p. 255 all the observations which leave the matter doubtful are given as proving vertical polarisation, and about the one contradictory observation we are told that "it must be borne in mind that a half rotation of his instrument would be sufficient to reverse the position of the colours."

We are then informed that vertical polarisation is exactly what "we should expect." But in a footnote on page 330 we are treated to some vague considerations, from which it "evidently" appears that the polarisation ought to be horizontal or exactly opposite to what we were first led to "expect." And now we are informed that out of eight observations which have been adduced to prove the first statement, seven prove nothing, and the observation which we first had to set aside on account of the "half rotation" is now treated as the only good one. Of the one observation which contradicts this new result we are told that "it seems, perhaps, more natural to assume that he commenced his observations with the polarimeter in a vertical rather than in a horizontal position."

Mr. Ranyard adds that this note must be taken as overriding the opinion expressed on page 255.

What we should "expect" is not at all so "evident" as Mr. Ranyard makes it out to be. If we confine ourselves to clouds, as Mr. Ranyard does, or to the reflection of sunlight from the surface of the illuminated parts of the earth, the polarisation would altogether depend on the distribution of the clouds, it would in all cases be very small, and a small change in the distribution might change the polarisation from one plane to the other. The polarisation due to reflection from unobserved parts of the earth or clouds uniformly covering it, ought to be vertical as we might "expect," in Mr. Ranyard's first statement, and not horizontal, as is "evident" in his second. If the clouds are higher up a different result is possible. There is, however, a second and much more powerful cause of polarisation which is due not to clouds, but to particles which scatter the light without polarising it, but are not sufficiently dense to make the medium opaque or to form a cloud. It is well known that in the neighbourhood of the sun the light is always polarised in a horizontal plane, and the cause we have mentioned seems to be the only one which can account for the facts. Exact observations during total eclipses would be of great use, and might throw some more light on the subject, which has by no means been fully cleared up. A great deal more may be said on this question, but we have only made the foregoing remarks in order to show that the subject cannot be treated in the short offhand way adopted by Mr. Ranyard.

Our second remark refers to a somewhat clumsy mathematical investigation. Mr. Ranyard investigates the polarisation of the light scattered from an atmosphere of particles uniformly distributed within a spherical shell surrounding the luminous point. We may mention that no conclusions whatever can be drawn from this problem with regard to the corona. One of the supposed results, however, of the investigation is that the light coming to us in the direction of the luminous point itself is polarised, and on this result Mr. Ranyard remarks—"At first sight it might appear that at the central part of the corona the light should be entirely unpolarised, but it must be remembered that the illumination of the particles adjacent to C will be very great, and the polarisation of the light dispersed by them will, it appears, overpower the non-polarisation of the light dispersed by the higher parts of the corona." In this passage the atmosphere of particles referred to above is called the "corona," and C is the luminous point. Had Mr. Ranyard ever asked himself the question in what plane the polarisation in the central part of the corona ought to be, the above passage could never have been written, for, reasons of symmetry would have been sufficient to show that no such polarisation is possible. Nor is the reason why the formula breaks down in this particular case far to seek.

We turn with pleasure to the chapter on spectroscopic observations, not that we consider this chapter faultless, but because in it we have brought before us a greater array of facts and not so much theory. For any one who in future eclipses intends to follow up the results achieved with the spectroscope by previous observers, this chapter will be invaluable. In no case is the comparison of different observations of such importance as in spectroscopy, and we are glad to have before us all the evidence, positive and negative, on a great many partly still undecided questions. The last and longest chapter is in some respects the most interesting. In it we have a careful comparison of the more important drawings and photographs of the corona, made during a long series of eclipses. The symmetry of the corona about an axis, nearly coincident with the solar axis, cannot be doubtful any longer, but whether the axis of symmetry is really the sun's axis or only approximately coincident, is one of the important points which will have to be decided during future eclipses.

At the November meeting of the Cambridge Philosophical Society in 1878, I drew attention to some remarkable changes which the outline of the solar corona had undergone during recent years; the change seemed to be periodical, and the period seemed to be that of the sun-spots. In the *Observatory* for December I also made some remarks to the same effect. Pp. 496–501 of the volume before us contain short descriptions of the general shape of the corona during all the eclipses, of which we have drawings, and in all cases information on the number of sun-spots is given.

The result seems to be altogether in favour of such a relation between the two phenomena as I have pointed out. In the papers above referred to, as well as at the November meeting of the Astronomical Society in 1878, I also pointed out a still more remarkable, though perhaps more doubtful, coincidence. It is generally found that the symmetry of the corona is not complete, but that one side is nearly always more fully developed, while the other is more contracted; and this departure from symmetry seems to be related to a direction fixed in space. Thus the eclipses of 1868, 1874, 1875, 1878, which all happened near the minimum of solar activity, resemble each other in general character; but in the eclipses of 1874 and 1875 the east[1] side of the corona was much wider than the west, while in 1868 and 1878 the west side was the broadest. Now the eclipses of 1874 and 1875 happened in April, while the eclipses of 1868 and 1878 happened in August and July. The eclipse of August, 1869, resembles also the two last mentioned in this departure from symmetry, and the eclipse of December, 1871, does not show any decided difference either way. If this remarkable connection of the general outline of the corona with the heliocentric position of the earth at the time of the eclipse should be confirmed, it would tend to show that the outer corona at any rate is of cosmic origin, and this view would be supported if it should be found, as is probable, that the axis of general symmetry of the corona is not exactly coincident with the solar axis. The connection between the sun-spots and corona would, according to this view, be reversed; that is to say, the more disturbed state of the corona at the maximum would not be produced by the increase of sun-spots, but the sun-spots would ultimately be produced by a cosmic cause which has the same period. The next eclipse in 1882 will not unfortunately throw much light on this question, as it will most likely resemble the eclipse of 1870 or 1871.

The great value of the book before us lies in the many suggestions for future observation which offer themselves on its perusal. It teaches some lessons which all of us who have ever observed eclipses or are likely to observe them in future would do well to remember. Thus on reading over the different and often conflicting accounts of one and the same phenomenon, we are struck with the insufficient descriptions of the apparatus and instruments with which the observations have been made. We are told by an observer what he has seen and what he has not seen, but on trying to compare his account with that of others, we generally miss some important information. Observers with the spectroscope, for instance, generally do not tell us anything, or at any rate they do not tell us enough, about the width of slit which they have used and about the luminosity of their spectroscope. Observers with the polariscope speak of bands, vertical or horizontal, and their description generally fails where it is most wanted, and we cannot decide whether a certain polarisation has been observed, or one at right angles to it. We ought, however, to exempt the Washington observers from this general condemnation. The reports emanating from the United States Naval Observatory are generally a model in the accuracy of their accounts.

The preparation of the eclipse volume has necessarily taken a good many years, and it would have been useful if some information had been given as to when different parts of it have been written. The Report of the Eclipse of 1875, for instance, appeared while the work was progressing, and it is natural, therefore, that only those observations could be mentioned which referred to subjects treated in the last few chapters. Yet some information might have been given that the treatment of that eclipse is only incomplete. The remark on p. 373 can only have been written before Mr. Ranyard could have known anything of the results which had been achieved.

As a book of reference to those interested in eclipse observations, the present volume is, as we have already mentioned, of very great value. Our thanks are due to Mr. Ranyard for the great trouble he has taken in its preparation. The blunders contained in it are so obvious, as a rule, that they will hardly mislead any one who is likely to make use of the book. ARTHUR SCHUSTER

NICHOLSON'S TABULATE CORALS

On the Structure and Affinities of the Tabulate Corals of the Palæozoic Period, with Critical Descriptions of Illustrative Species. By H. Alleyne Nicholson, M.D., D.Sc., &c., Professor of Natural History in the University of St. Andrew's. (London and Edinburgh: W. Blackwood and Sons, 1879.)

THIS handsome post octavo volume contains a disquisition on a rather heterogeneous assemblage of organisms partly Cœlenterate and partly Bryozoan in nature, partly of uncertain affinities. Of the palæozoic Cœlenterate corals described, the major portion are probably Alcyonarian. None of them, except perhaps Labechia, appear to belong to the Hydroids, the family of the Hydrocorallinæ being, as far as has yet been discovered, of comparatively recent origin, and not of older date than cretaceous deposits at the most, unless, as believed by Mr. Carter, Stromatopora and its allies of the Silurian formations are in reality closely related to Millepora.

Prof. Nicholson, in the recent edition of his "Palæontology," places Stromatopora amongst the sponges, and does not accept Mr. Carter's conclusions. He seems in doubt about the true relationship of Labechia from the Upper Silurian, the similarity in the structure of which to the Milleporidæ was first pointed out by Dr. Lindström. The author himself, in conjunction with Dr. Murie, was one of the first to investigate the finer structure of Labechia, and described his results in a memoir on the Stromatoporoids, in the *Journal* of the Linnean Society.

The present work commences with a short review of all those corals which possess tabulæ or transverse calcareous partitions within their pores or calicles, and which were formerly grouped together by MM. Milne Edwards

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[1] Owing to a slip it was said in the Report on the Eclipse of 1875 that the west side was the widest in 1874 and 1875.